form of a spongy or cellular texture, and in that of ramified tubes in the vessels. Any considerable diminution of the aqueous element of the blood must occasion an exhaustion of this great sac, and be attended with a tendency to a vacuum, occasioning a collapse or shrinking and shrivelling of the solids in the interstices, and, it is by no means an improbable supposition, between the molecules of which this vascular tissue is arranged, conveying the supplies destined for their renovation. The fulness, plumpness, and elasticity of the solids, are owing to the presence of this element in their texture—it in fact forms a very large portion of the constitution of the organs, as is proved from the great loss of weight produced by desiccation, and it is necessary to the perfection of their properties.

In another point of view, the aqueous fluid, as an element of the blood, is not less important and essential to its natural and healthful constitution. It is the vehicle or medium by which the solid portions of the blood—the globules, fibrin, and colouring matter are transported and conveyed into the interior and intimate structure. The mobility of the blood, and consequently its fitness for circulation, is wholly derived from its aqueous portion, and if this be largely diminished, the movements of the blood are embarrassed, enfeebled, and finally cease, from the viscidity of the sanguine fluid depriving it of the power of motion. To the preceding phenomenon of collapse proceeding from the exhaustion or emptiness of the angeal or vascular cyst or sac, is added the loss of the circulating movement of the blood.

The foregoing preliminary principles became indispensable to a correct appreciation of the phenomena distinctive of the second group of cases composing the third and fourth periods of cholera, but our remarks have extended to so great a length that we must postpone the investigation of these to the succeeding No. of the Journal.

Art. II. *Case of Malformation, with Remarks on the Circulation of the Blood.* By Thomas Robinson, M. D. of Petersburgh, Virginia.

EARLY in September, 1828, while confined to bed with a remitting fever, I received an urgent summons to visit a lady whom I had seen some days before in an advanced state of pregnancy—the messenger was also desired to say that something very extraordinary had happened—this hint roused me. I dressed and repaired to the house as speedily as possible. On entering the chamber, a full grown infant born about twenty minutes before, was presented to me. I was informed that it had struggled strongly for some minutes after its birth, and gasped frequently—the head was finely developed, the face beautiful, the trunk and extremities large and plump, except the left arm, the elbow of which was confined close below the inferior edge of the scapula, the forearm closely flexed, and the whole limb covered by the integuments of the trunk, except the hand, which appeared at the edge of the pectoral muscle—both clavicles, the sternum and cartilages of the ribs were wanting, exposing the whole interior of the cavity of the thorax: as the shoulders and ribs were thrown back in a remarkable manner, being unconfined by clavicles, the abdomen was also open as low as the umbilicus, to which point the margins of the chasm converged from the second false rib at each side; along the right margin, the umbilical cord appeared to ramify, and there to expend itself—the investigation of this part, however interesting, was not pursued. At the right side, close within the verge of the opening and about midway between the umbilicus and rib, a body about the size, colour, and shape of the gizzard of a large fowl presented itself; this mass attached to the margin was hard and *perfectly fibrous*, well furnished with blood-vessels, and communicated by a small branch with the funis; it had no communication with the duodenum, no trace of gall-bladder, and in no particular resembled the liver—the liver and spleen were wanting; the diaphragm cleft, a narrow strip at each side being barely cognizable; the lungs were wanting, the trachea terminated about the place of the first dorsal vertebra, in a knob of cellular and parenchymatous substance not larger than a hickory nut; at the left of *the spine and higher in the thorax than usual*, was seen the heart without a pericardium, it was placed very obliquely as well as high up, so that its apex could not have struck below the third rib; it acted with surprising force and regularity between 60 and 70 times in a minute, and appeared to carry on the circulation, as the aorta pulsated very distinctly; my whole attention was immediately riveted on this unexpected phenomenon—as I am one of those obstinate people who will always see things as they appear to their own senses, and understand them according to the dictates of their own reason, I have been long puzzled on the subject of the circulation, by the discrepancies of facts and theories exhibited by systematic writers. Whenever I have found it difficult to adapt plain phenomena to preconceived notions, instead of straining the facts to fit the notions, I have unceremoniously discarded the notions and retained the facts; this is at least a safe practice: in the case before us, I shall first endeavour to describe with scrupulous exactness, the action of the heart exhibited to my own sight and touch, and I think I can afterwards satisfy you, that the same phenomena have been seen and felt by others, and rejected as incompatible with some favourite theory.

Viewing the heart and feeling it during the pause, although it was perhaps less hard than in either diastole or systole, yet it did not appear in that state of complete relaxation which physiologists ascribe to it; perhaps the antagonizing powers were merely in equilibrio; possibly the contracting power predominated; from this state of quiescence it would suddenly spring dilated with surprising force, its apex elevated with a jerk, finish its double action in the twinkling of an eye, and fall back as suddenly to the stillness of death; no motion like sudden or gradual relaxation appeared in its fibres, though vigilantly watched for, and indeed expected; the diastole, which always preceded, appeared to commence in the venous sinus, and pass without interruption and with immense velocity to the apex; the systole pursued the same course with equal velocity. When you consider that this double action was completed in a space of time less than half a second, perhaps not more than a third, for I think the time of the pause nearly, if not quite double the time of the action, you will readily comprehend the difficulty of noting particulars in their exact order; that the action passed from base to apex was visible enough—also that the diastole preceded the systole; but so rapidly did the systole succeed the diastole, that I could hardly ascertain with perfect satisfaction to myself, whether the diastole of the ventricle was *entirely* completed before the systole of the auricle commenced; my decided impression however is, that at one and the same moment the diastole is finished, and the systole commenced. It may here be asked, if the diastole continues for so brief a moment, is there sufficient time left for the blood to occupy the cavities? I would answer, were dilatation a passive movement, it would certainly require more time for the pressure of the blood to overcome the vis inertiæ of the heart, and dilate its cavities, but when we consider that the blood is accumulating in the adjacent vessels during the pause, and under strong pressure, and when we estimate the great force and velocity with which the cavities dilate and the consequently powerful suction they exert, we can hardly doubt that they are *instantly* filled; at the moment of diastole the apex of the heart was elevated; at the moment of systole it fell back with great suddenness. After watching the heart in situ fifteen or twenty minutes, and frequently grasping it to ascertain its force in dilating, I separated it from its connexions; its action continued without sensible diminution, either in frequency or force, and in that state it was put into the hands of different individuals, that they might be qualified to testify the fact. I then proceeded to examine the state of the valves and septum, which were found as usual; the foramen ovale open, the pulmonary vein and artery, cava and aorta of the proper size; in this state, with auricles and ventricles laid open and roughly handled, the heart was thrown into a basin of cold water; after examining the other viscera for some time, I returned to the heart, and was surprised to find it still moving, feebly it is true, but with perfect regularity; the attention of the spectators was again directed to this unexpected state, and it was again placed on the hand of Mrs. F. who attended the accouchement. At this period I felt myself sinking so fast, that I was reluctantly obliged to retire without inspecting the viscera of the pelvis, tracing the lesser circle of circulation, or even injecting the funis, the course of which I could not even conjecture without such aid. In a few brief propositions, I will here recapitulate the principles which this interesting case has fully established in my mind.

1st. Systole and diastole are the natural muscular actions of the heart.

2d. The force of the diastole is equal to, if not greater than the force of the systole.

3d. There is no pause between auricle and ventricle, either in diastole or systole; the action passing from base to apex with great velocity; *perhaps* accelerated, *certainly not* retarded.

4th. There is not only no pause between the diastole and systole, but the latter almost seems to appear before the former vanishes; thus presenting the appearance of one compound action in which all the parts concerned coöperate perfectly.

5th. The diastole always precedes the systole in the natural order of action. BICHAT's experiments led him to the same conclusion.

6th. Similar actions in similar parts of the different sides are always synchronous.

7th. The time of action, including systole and diastole, is less than the time of the pause.

8th. The pause succeeds the systole of the organ.

9th. The impulse of the apex against the side of the thorax is caused by the diastole of the auricles, especially the left—for as the distance of the axis of the heart from the diaphragm must be increased by the increase of its transverse diameter in diastole, and as it is firmly tied down at its base, this increase of distance of the axis can be effected only by its turning on the distended auricles—as each action of the heart is performed by a convulsive jerk, the force of the impulse is naturally accounted for.

· 10th. There is in the heart a vis insita or vita propria; this is demonstrated by its perseverance in action so long after its entire separation from all influence of brain, nerves, and ganglions.

The system of action deduced from this case, exhibiting a perfect coöperation of the whole organ in each action, and concurrence in repose, seems much more simple and consonant to nature than the complicated series of alternations so zealously inculcated by the ancients, and which, with but very little modification, has kept possession of the schools to the present day: passive diastole of auricles—active systole of auricles—passive diastole of ventricles—active systole of ventricles: in this series we are presented with a perpetual union of action and inertia in the same organ—one-half being always asleep, and part of the power of the waking half continually wasted in overcoming the inertia of the sleeping half; this I never could receive; but after witnessing the simple series of nature, diastole of the whole organ, systole of the whole organ, repose of the whole organ, the former arrangement appears only as a clumsy, ridiculous contrivance.

Most of the errors connected with the subject of the circulation, from Harvey to the latest writers, may be traced to one and the same source, that unaccountable delusion, that during its diastole the heart must be in a perfect state of inertia. "Fibris enim propriis dilatari repugnat oculo." (Haller, prim. lin.) "Neque relaxatio cordis aliquarum fibrarum actio naturali est." (Coroll. de cord. mot.) "Diastolen nullius strati musculosi opus esse, sed relaxationem et meram inertiam." (De sang. mot. ejusq. causis.) Consequently the inert auricles must be dilated by the pressure of the blood from the contiguous veins, and consequently power lost—they must be stimulated to contraction by the blood forced into their cavities—the whole of the force with which they contract must be wasted in dilating the torpid ventricles, which in their turn must be stimulated to contraction by the fluid forced in, and by no other cause—"Non aliam causam esse hujus in motu pertinaciæ præter ipsum perpetuum sanguinis adfluxum." (De cord. mot. a stimul. natur.) Thus, from a single preconceived error a series of absurd alternations have been ascribed to the heart. "Ut auricularum contractio cum ventriculorum relaxatione, ventriculorum systole cum auricularum diastole conjuncta sit." (De mot. sang. per cord.) A system repugnant to nature, to reason, and to the recorded observations of its advocates—a system by which the heart is deprived of about three-fourths of its power as an agent in circulation; for, by denying the power of the diastole, you deny at least half the power of the heart; and as the whole systole of the auricles is wasted on the diastole of the ventricles, that too must be deducted, leaving only the systole of the ventricles.

Such is the system of Harvey and his successors:—"Hæc in cordis motibus series Harveio quidem placuit omnibusque anatomicis ab eo qui viva animalia operuesunt." (De mot. sang. per cord.) Although so many alternations successively performed *in one second*, would seem to leave little time for pause, yet I believe all physiologists, ancient and modern, concurred in the belief of at least an apparent pause, after the systole of the ventricles. "Expulso sanguine cor quiescit ex solo stimuli defectu" has been generally received as an axiom till the time of LAENNEC—he supposes, that the heart pauses while the ventricles are subjected to the double stimulus of distention and of the blood; this error, for pace tanti viri error it is, grows naturally out of the theory under review. Laennec, in exploring the thorax, hears two sounds in rapid succession, followed by an interval of silence, marking, as he supposes, the pause of the heart; the first sound is accompanied by an impulse felt in every part of the arterial system; this impulse is probably the cause—it cannot proceed from the systole of the auricles whose impulse cannot reach the arterial system, it must therefore be ascribed to the systole of the ventricles; the second sound must have some active cause—diastole is passive, therefore silent—the only remaining cause then, to which it can be ascribed, is the systole of the auricles. This course of reasoning seems to lead more directly than any other to Laennec's conclusion, which after all is a non sequitur; for I more than suspect, that the true cause has been omitted in the enumeration; besides, the premises are all false. MAGENDIE admits that the heart is not entirely passive in diastole; BICHAT affirms that it is powerfully active, and the case before us confirms the fact—the diastole then is as likely to be heard as the systole.

That both systole and diastole of the ventricles are uninterrupted continuations of similar actions of the auricles, seems also to be established; their sounds, if any, must therefore be indistinguishably blended.

If the auricle and ventricle coöperate in the diastole, as I think I have seen them do, there is then a continued column of blood from the venous to the arterial valves; consequently, the first impulse of systole at the base of the auricle is felt through the whole arterial system. Again, considering the diastole of the auricle as the cause of the impulse of the apex against the side, (Prop. 9th,) and remembering also the celerity with which the systole follows it, (Prop. 4th,) I think the impulse of the apex against the side, and the commencement of the systole in the base, must be so nearly synchronous that no variation can be distinguished by the senses—the vibration generally perceptible in a strong pulse, may be ascribed to the increased impetus from the superior power of the ventricle. Haller, after ascribing the stroke against the side to the systole, and especially to the curve given by that action to the apex, is obliged in candour to acknowledge that there is a further cause, to wit, the diastole of the left venous sinus—"Antrorsum *preterea* pulsus a sinu sinistro venoso, qui eo imprimis tempore repletur." (Prim. lin.)

If this view of the circulation be correct, and I at least must believe that it is, wo ought to proceed without delay to review our system of diagnosis in cardiac diseases, more especially that part which depends on the stethoscope, (I would fain call it stethacoust, as I do not *see* but *hear* through the instrument.) I hope you will not suspect me of attempting by any of my remarks to depreciate the invaluable labours of Laennec, to whose sagacity and industry our profession is so deeply indebted. On the subject of his errors I would only say—"nonnun quam bonus dormitat Homerus."

That Haller and many of his predecessors, contemporaries and successors, who practiced vivisections, saw the very same actions of the heart which I have described, but explained them to suit preconceived notions, might be proved by numerous quotations. They saw and felt the heart dilate with force. "Observavimus aperto pectore et pericardio, &c. &c."—"prementem digitum *valde* repellere." (Stenon. act.) "Cordum pulsat, digitum repellit non sine robore." (Haller passim. de mot. cord.) Can any one believe that the finger was repelled by such a *contraction*—"ut basis apici, ventriculus ventriculo fiat proprior"—the organ literally shrinking in all its dimensions would have receded from the hand: would not forcible dilatation have been a more plausible agent of strong repulsion? but the heart is passive in dilatation, therefore the finger was repelled by contraction. They saw the heart dilate and contract from the energy of its own innate power, without mechanical distention or stimulus. "Motus cordis non turbatur capite resecto neque corde avulso." (De mot. cord.) Hundreds of similar observations on this point might be collected from various authors. They saw the same blended action of diastole and systole, as I have described, as even Harvey acknowledges, (Exerc. de motu sang.)—"In calidioribus animalibus motum cordis et auricularum videri quale simul fieri." Haller expresses himself to the same effect in the following passage:—"Dum alacriter auriculæ micabant intervallum quo earum motus præcedit cordis motum diu ægre distinxi *donec animal vegetum fuit.*" (De mot. cord.) Again—"Et *tamen candosis mei est*, phænomenon non *dissimulare* quod ipse vidi et ante me Lancissius, omnino nempe credas te videre, eodem tempore, et duas auriculas constringi et duos ventriculos." (De mot. cord.) Haller in this passage acknowledges that the facts he states are inconsistent with his theory; on this ground LANCISSI arraigns the series of Harvey, and proposes one perhaps nearer the truth, but still defective. I hope I have by this time convinced you that I saw nothing new in this case, and can therefore claim only the vulgar merit of crediting my own senses.

A strong argument in favour of the series of actions suggested by our case, may be founded on the structure of the organ itself. Who ever inspects with candour and attention the structure of the valves, will find it difficult to persuade himself that they are adequate to the function generally assigned to them; he may easily ascertain that they offer an *impediment* to the reflux of the blood, but hardly that they afford a complete obstruction; but as the contraction of the auricles is less powerful than that of the ventricles, such reïnforcement seems necessary to prevent reflux.

Of this imperfection of the valves Haller was convinced by the inspection of living animals, and remarks, "Non adeo fidelem valvularum custodiam esse." (De mot. sang. ejusq. caus.) Again he says, "Neque adeo in vivo animali, tantam esse valvularum efficaciam ut vulgo creditur." If such is the inefficacy of the valves, suppose the auricle in diastole and ventricle in systole, (according to the received opinion,) the reflux must seriously diminish the quantity of blood sent to the arterial system; but suppose with me, that the auricle is in a state of contraction, the only safe-guard against reflux at this time, then will the whole of the blood contained in the ventricle be transmitted by its systole to the arteries; the auricle being less firmly guarded in this way, we perceive a reflux at every contraction, which however is checked by the column advancing in the veins; this simple explanation of the reflux appears less objectionable than that of Magendie.

While Bichat affirms that the heart dilates with a force which no effort of the hand can prevent, is it not surprising that he has neglected to apply a power so great and so obvious to the elucidation of that obscurity of the venous circulation, on which he acknowledges authors have hitherto shed few rays of light; to you it is unnecessary to demonstrate that the pressure of the atmosphere on the veins external to the cavities must be propagated to the termination; that the cavities being always full, there is a continued pressure on the vessels within them; add to this the powerful dilatation of the heart, auricles and ventricles coöperating in the action, as I have seen them, and you have all the requisites of a powerful sucking pump operating perpetually on the venous system. Dr. James Johnson, whose suggestions are always worthy of attention, has long declared his belief in the existence of such a power.

That the heavy mass of blood contained in the inferior part of the trunk and lower extremities could be elevated to the heart of a tall man standing erect, by the sole force of insensible contraction of the capillaries, (as Bichat supposes,) is so startling an hypothesis, that were I obliged to say *credo* to it, I should certainly add in the words of the old churchman, "*quia impossibile est:*" besides, this insensible contraction of the capillaries has not been demonstrated; the pump I think has been, and that its agency in the venous circulation is more powerful than that of any other cause yet assigned, or than the combined powers of all the other causes, I have no doubt; other causes not immediately connected with our case I shall pass by with one exception. Dr. Barry, in one of the loosest and most illogical essays that ever obtained the approbation of a learned institute, and in which he betrays a marvellous deficiency in pneumatic and hydraulic science, has attempted to show that the venous circulation is greatly promoted by inspiration; the blood he thinks rushes through the veins towards the cavity of the thorax, to fill the vacuum occasioned by the elevation of the ribs; he forgets that the air also rushes towards the opening vacuum, and that if two fluids, one of which is highly subtle, the other dense and viscid, present themselves under the pressure of the atmosphere at equal apertures, to an opening cavity, the quantities of these respective fluids, entering in a given time will be found in the increase ratio of their density and viscidity. If Dr. B. has any doubts of this truth, as he is fond of experiments, let him procure a large syringe furnished with two pipes of equal diameter, one being curved; let him immerse the aperture of one in fluid blood, or any other fluid equally dense and viscid, the other remaining in the air—then draw back the piston and ascertain the ratio of air to the other fluid in the machine, provided he finds any other; afterwards let him compare the lax, inelastic, long, tortuous vessels, through which the venous blood has to pass, with the wide, elastic, short, patulous pipes which admit the air. Should he afterwards demand the advance of more than half a drachm of blood for each inspiration, I shall consider him a very unreasonable person. Were it necessary at this day, it would be easy to show that the venous circulation is on the whole retarded by respiration.